The Effectiveness Review Trials of Hercules and Some Economic Estimates for the Stables

Ted R. Miller, PhD, MS

The literature on motor vehicle safety is vast. Consequently the review effort reported in this supplement was Herculean in scope and difficulty. It introduced me to many solid and important effectiveness studies. At the same time, it occasionally omitted effectiveness studies that I cite. Returning to my sources heightened my appreciation of the Task Force on Community Preventive Service's (the Task Force) trials. The first trial was finding the studies. Two examples are informative. A National Center for Health Statistics publication[1] finds that 92% of low-income parents who own child safety seats use them routinely. That report, however, covers a wide range of parental safety practices. It lacks keywords and is not indexed. Again, an article in an economics journal[2] uses confidential 1983 National Personal Transportation Survey microdata to analyze how people make decisions about using motor vehicle safety equipment. The paper includes a logit regression explaining child seat use. One explanator is residence in a state with a child safety seat use law (in force in 1983 in 15 states housing 38.5% of the 934 respondents with children under age 5). The model focuses on the influence of individual factors like parent age, income, and education on seat use decisions, but in the process it produces the best extant evaluation of the impact those laws had on seat use. It finds that laws increased seatbelt use by 42.3%, with 17.7% diverted from belts and 24.6% restrained for the first time. These findings, however, are by-products. They do not appear in the abstract and merit only one sentence in the text. To the author, a restraint law was just another regression coefficient. How could a systematic search find these studies?

The Task Force's second trial was evaluations measuring different outcomes of comparable interventions. Despite many sound evaluations, the number using any single measure sometimes was dangerously small. Seemingly anomalous meta-analytic effectiveness estimates sometimes resulted. Most notably, when most child seat laws passed, child seat effectiveness was about 54% against fatalities and 52.5% against nonfatal injuries.[3] So child seat laws should decrease deaths and injuries proportionally and by roughly half the amount that use increases. Yet, in the studies reviewed in Table 2 of Zaza et al.,[4] laws decrease deaths and injuries combined by 17.3% but decrease deaths alone by 35% and increase use by just 13% (24% if we add the Blomquist study[2]). By dividing deaths and injuries by effectiveness, we can convert the estimates to compatible units. Doing so reveals that one study, which found 57.3% effectiveness against fatalities, must have been analyzing effectiveness among seat users (or else laws brought 100% seat use). Across the remaining studies including Blomquist, it appears that use increased by 35% at the median, reducing deaths and injuries by 18%.

The third trial, which the Task Force handled extremely well, was co-mingled interventions. States do not legislate for the convenience of evaluators. Especially when attacking impaired driving, they often simultaneously implement a package of interventions. The Task Force had to reject some otherwise sound evaluations because studies either could not separate the effects of packaged changes or attributed improved outcomes to a subset of the actual package. It is unclear if we even should try to separate impacts of package components. Synergy may heighten their yield.

The fourth trial was meritorious effectiveness estimates that could not be converted to the Task Force's chosen effectiveness units using only information in the articles. The necessary information occasionally was available from other publications or by contacting the authors. I wish the Task Force had been able to salvage those estimates. Well-designed studies are rare enough that we need every one.

The fifth trial was time-dependent effectiveness. The effectiveness estimates for child safety seat community information/enforcement and education/use incentives come from years when most parents did not use child seats. Today, people know that child seats are a part of good parenting. It seems unlikely that these measures would have nearly as much impact now that both national observation surveys and parent interviews suggest that use exceeds 90%. Similarly, it would be useful to assess the impacts of enforcing safety belt laws as a function of pre-enforcement usage rates.

The sixth trial was inaccurate police reporting. Police

From the Pacific Institute for Research and Evaluation, Calverton, Maryland

Address correspondence to: Ted R. Miller, PhD, MS, Pacific Institute for Research and Evaluation, 11710 Beltsville Drive, #300, Calverton, MD 20705. E-mail: miller@pire.org.

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reports code 26% of the injured as uninjured, but 12% of people coded as injured are not.[5] These errors vary considerably among states and sometimes are predictable consequences of definitional differences.[6,7] Thus, state-specific estimates of the impact on police-reported injuries, especially serious injuries, may differ because of definitions. Meta-analysis averages these inherently inconsistent estimates. Again, comparisons with blood alcohol measurements by medical personnel show that police do not code alcohol involvement in 26.9% of injury crashes with driver blood alcohol concentration (BAC) above 0.10, and 54.6% with lower positive BACs.[8] Adjusting the estimates in Table 2 of the article by Shults et al.[9] accordingly with data on miles driven by BAC[10] yields estimated probabilities of 67% in late-night, single-vehicle, nonfatal-injury crashes and 60% in all late-night nonfatal-injury crashes. Those percentages are virtually identical to the 64% and 60% in fatal crashes. They cast a different light on the proxy measure assessment done by Shults et al.[9]

The final trial was the troubling dearth of economic analyses. The important question is why so few exist. These analyses could be done in two ways—as part of an intervention evaluation or by applying effectiveness data and separately modeled data on the injury costs that can be avoided. Because of the expense of collecting cost-savings data, the latter approach is preferable. The necessary injury cost data have been published.[11] So why not cost-outcome analyses? Because no intervention cost data exist! Published program descriptions and effectiveness evaluations almost never report costs of program implementation or replication. To learn the cost-effectiveness of programs, we need to change that. Peer reviewers and journal editors need to push for cost data, perhaps even by making program cost a category in structured abstracts describing interventions.

Economic Analysis

The rest of my comments add economic analysis data, review problem size, and estimate benefit–cost ratios and costs per quality-adjusted life year (QALY) saved for the interventions. This is based on the methods in my review of 84 cost-outcome analyses in injury prevention and control,[12] presenting estimates in 1997 dollars for comparison to that review's estimates, and sometimes refining its estimates.

Table 1, drawn from a recent article,[11] summarizes the cost impacts of the problems examined by the Task Force. Alcohol-impaired driving is the largest problem, followed closely by unbelted driving. These problems dwarf those of impaired driving by underage drinkers and especially child seat non-use. The analyses use these data and unit costs from the same article to value benefits. They also use the child seat benefit-cost information in an article[13] that evaluated a more generic child seat intervention than the ones the Task Force analyzed. The analyses examine societal savings. The measures evaluated are described in the Task Force reviews.

Table 1. Costs of problem behaviors in 1993 (in millions of 1997 dollars)

	Impaired driving	Impaired driver under age 21	Child seat non-use	Belt non-use
Medical	5,161	1,206	89	4,136
Other monetary	35,568	7,312	488	29,117
Quality of life	59,752	13,777	1,561	61,686
Total	100,481	22,295	2,138	94,939

Source: Miller et al.,[11] inflated to 1997 dollars.

How can we estimate intervention costs, given that none were published? For laws and regulations, we can use Downing's[14] estimates that the costs of approving mandates average 2.9% to 7.1% of the first-year direct costs imposed on the public, with public implementation and administration costing another 4.2% to 4.6%. This means that total costs will average 9.4% more than first-year direct costs.

A child seat law increases seat use (and presumably seat purchase) at a retail price around $45 per seat. Thus, per seat purchased, a child seat law costs $49 ($45 × 1.094). Telephoning staff at the National SAFE KIDS Campaign and a few active local program operators suggests that child seat distribution programs distribute seats at $45 or less. This estimate includes the cost of counseling on correct use. The benefits analysis[13] accounts for non-use by seat owners.

Forced belt use imposes temporary discomfort and inconvenience costs estimated at $22 per new user per month.[15] I assume that these costs persist for 6 months, then fall to 10% of their prior level as people get used to buckling up. Costs and return on belt laws are evaluated over the first 5 years post-implementation, with Downing's factors applied to first-year costs. Since the costs and benefits of belt law passage both are linear functions of the number of new belt users, passing a seat belt law and changing a secondary law to primary have the same benefit–cost ratio. Belt law enforcement adds travel delay costs to the discomfort costs. Usage checkpoints typically delay vehicles for 5 minutes. With 17% short-term effectiveness and 14% long-term,[16] North Carolina's checkpoints per registered vehicle, the U.S. average of 1.5 occupants per vehicle, and delay time per occupant valued at 50% of the wage rate,[17] delay costs add $0.83 to the cost per new belt user. The Downing factors suggest that an intensive belt-use enforcement program would cost $485 to $800 million to implement nationwide, with the lower end of the range more probable since only administrative action is required. Confirming this estimate, costs would be $570 million with police costs per belt use checkpoint equal

Table 2. Benefit–cost ratios and costs/QALY for selected highway safety measures (in 1997 dollars)

	Unit cost	Benefits (costs averted)				Cost ratio	Benefit–cost/QALY[c]
		Medical	Other monetary[a]	Quality of life	Total benefits[b]		
Child safety seat law	$49/seat purchased	$100	$360	$1,000	$1,500	31	<$0
Child safety seat distribution	$45/seat distributed	$100	$360	$1,000	$1,500	34	<$0
Pass a belt law	$260/new user	$180	$1,260	$2,670	$4,110	16	<$0
Upgrade secondary law to primary	$260/new user	$180	$1,260	$2,670	$4,110	16	<$0
Enhanced belt law enforcement	$240/new user	$150	$1,030	$2,170	$3,350	14	<$0
.08% driver blood alcohol limit	$2.70/driver	$2	$13	$22	$38	14	<$0
0-tolerance of alcohol, drivers under 21	$29/driver	$38	$210	$400	$650	22	<$0
21 minimum legal drinking age	$150/youth 18–20	$27	$160	$300	$490	3.2	$18,000
Sobriety checkpoints	$8200/checkpoint	$3,400	$15,100	$37,000	$55,500	6.8	<$0
Mandatory server training	$59/driver	$10	$71	$120	$200	3.4	$16,000

[a]Monetary costs include direct nonmedical cost savings as well as indirect work loss savings. Cost/QALY = QALYs saved/(intervention cost-direct cost savings)
[b]Numbers do not correspond exactly to prior columns due to rounding. All numbers were computed, then rounded.
[c]Cost/QALY, <$0 means the intervention offers net cost savings.
Note: These estimates can be compared to the 84 estimates in Miller and Levy[12] but supercede those estimates for belt laws and .08 blood alcohol limits. The 0-tolerance and sobriety checkpoint estimates come from the Task Force review.

to the costs per sobriety checkpoint net of breath testing equipment costs (about $6900)[18] and North Carolina publicity costs for belt use checkpoints of $0.5 million (F Smith, North Carolina Department of Transportation, personal communication, 1997) used as the average cost per state.

Assume that a .08 BAC law would cause impaired drivers to reduce the trips they would have taken with BACs of .08 and over by 6.5%, the same percentage as the decline in alcohol-related fatalities that resulted from these laws (the average of the two multi-state evaluations that separated the effects of administrative license revocation[9]). With miles driven by BAC in 1991[10] and the cost per mile of mobility loss,[12] costs of .08 are $2.70 per licensed driver (1.094 × .065 × 20,819 million miles × $.31 × 1.094/171.5 million drivers).

Similarly, assume that the 21 minimum legal drinking age (MLDA) reduced alcohol consumption among 18- to 20-year-olds by the same 19% as alcohol-related crashes.[9] Combining this information with National Household Survey on Drug Abuse data and alcohol sales data[19] suggests that sales declined by $1.48 billion (6.7% of consumption by youth aged 18 to 20 in 1996–1998 × $94 billion in alcohol sales × 0.19/0.81), so MLDA cost $150 per youth (1.094 × 1.48 billion/10.6 million youth).

Again, assume that mandatory server training decreased drinking over the legal limit while away from home by the same 23% that the Task Force reports it decreased alcohol-related crashes in Oregon. To compute the direct costs of server training, we multiply annual alcohol sales times the 40% of alcohol consumed by people who are over the legal limit times an assumed 75% of excess consumption that occurs outside the home times the 34.5% reduction times 1.094, arriving at a cost of $59 per driver. If implemented nationwide, Downing's estimate is that implementation and administration would cost $410-$450 million. Estimated directly in sensitivity analysis, with roughly 1.5 to 2 million alcohol servers nationwide, implementation cost would be a slightly lower $250 to $330 million.

Space does not permit deriving the benefits of each intervention, which came from the problem costs and the Task Force's effectiveness estimates. The computations tend to be straightforward. To give one example, the Task Force estimates that upgrading to a primary belt law reduces unbelted occupants by 14.1 percentage points from the 40% non-use level for 1993 in Table 1. Nationwide, upgrading would have saved $33.5 billion annually (94 × 14.1/40).

Sensitivity analysis revealed a curious fact. Unit purchase, alcohol sales reductions, and discomfort costs dominate the costs of these measures, with the remaining costs computed from them. Consequently, provided the items purchased or used are effective, the benefit–cost ratios and cost/QALY saved for child seat laws and giveaways, belt laws, .08 BAC laws, 21 MLDA, and mandatory server training are completely insensitive to their percentage effectiveness at changing usage, although net savings change linearly with usage. Applying this costing approach to laws mandating zero alcohol tolerance for drivers under age 21 would not make sense, however, because the alcohol is being sold illegally. The literature on crime suggests that criminals should not be considered to suffer losses—in this case lost sales—when their ill-gotten gains are cut off.[20] The same observation holds for the benefit–cost ratio for enforcing laws against serving intoxicated patrons,[12] a frequent companion to server-training programs. The difference in treatment of reduced alcohol consumption somewhat artificially makes law enforcement seem like a better investment than server training.

Table 2 shows the results. All the interventions yield large returns on investment, given that discomfort and inconvenience costs and reduced alcohol sales are included in the intervention costs. Even the measures with costs/QALY of $16,000 to $18,000 are attractive investments when judged by the criteria suggested in my review of 84 safety measures.[12] That is especially true since the 21 MLDA and server-training estimates are quite conservative. They exclude reductions in barroom brawls, vandalism, high-risk sex, and other adverse consequences of public drinking to excess.

Thus, economic analysis reaffirms the Task Force recommendations to adopt and maintain these interventions.

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